ON THE CONDUCTION OF HEAT.

Bridgend, March 9, 1843.

TO THE EDITOR OF THE BUILDER.

SIR,—With feelings of pleasure do I see in your paper of last week that your list of subscribers is daily increasing, and I, in conjunction with many others, do entertain the surest hopes of its success, as its exclusion of party politics renders it acceptable to all grades of persons who have the welfare of the country and its prosperity at heart; and I venture to predict, in a short time, it will be in as large circulation as any weekly paper now extant. And seeing that you have left its columns open to those persons who are kind enough to favour you with their correspondence on all questions which will tend to the advancement and instruction of its readers; such being the case, I beg to say that I, as an humble individual, will contribute the mite of what little information I possess, or my leisure hours will allow me, to its columns, and if it is of any value, you are at liberty to insert it. Then, Sir, I shall first begin with our present theoretical and experimental knowledge of the laws of conduction of heat, and to examine how far conclusions deduced from theory have been tested by experiment, as there is no reference to theoretical investigation, however important in itself, but is capable of being tested by a direct experiment; and the names of the many experimenters on this head I will pass over, as their experiments were not calculated to serve the test of theory. Then, to obviate this difficulty, I will arrange the matter under three heads: two of these are distinctly marked out by the object proposed to be effected, and the third is suggested by the consideration of the former two. Then, first, let us examine what is the present state of our theoretical knowledge of conduction: secondly, into the state of experimental investigation, so far as has been undertaken with a view to test or to illustrate the conclusions arrived at by theory; thirdly, the inadequacy of the few experimental facts with which we are furnished to serve either as the basis of a true theory or as the indication of a false one. I will therefore endeavour to point out, in conclusion, what are the most important results of theory which it is desirable for experiment to test, and to suggest a few of the most simple means of effecting the object desired. First, the problem, in the solution of which consists the mathematical theory of heat is the following:—Having the given state of heating, or the variation of that state from time to time, at one or more points of a homogeneous body of given form and dimensions, to find the permanent or variable temperature at every other point, thus: Suppose a ring to be kept at a certain temperature at one point, and it is proposed to discover, first, what is the variation from time to time of the temperature at every other point; secondly, what is the ultimate temperature to which any given point approaches, as the time the constant heating of one point has been kept up is increased. From this supposition it will appear that the answers to experimental facts, upon which the theory must rest, are answers to the following questions:—First, according to what law does a heated body lose its temperature to the air, or other medium or space by which it is surrounded? second, according to what law is temperature transmitted from point to point of a body? as on the correctness of the answers that may be assumed as given to these questions depends the applicability of the results obtained to the state of things in nature. Then I find that there are three distinct methods of theorizing, each adopted, apparently, in accordance with the known laws of nature, but which differ essentially from each other; and I do not perceive that our existing experiments bear with greater weight in establishing or disproving either of them — as the other two are confirmed by experiments each at variance with the other. I am not aware that it has once suggested itself to any one experimental philosopher to examine into the laws of conduction; but much labour, it is true, has been bestowed in examining the conductive powers of different substances, and to the result of experiments carried on with this object we look naturally with the hope of extracting a law; but unfortunately the nature of the experiments we are presented with is not such as will lead us to what we seek; as they are not originally conducted with reference to the state of things assumed to exist in theory, and are, in consequence, of less value when allowance is made between what they express and what theory requires. Now it cannot be denied that difficulties do attend the experimental examination of this subject, when it is intended to make every thing correspond with the state supposed in theory. The chief and greatest of these I consider to be the presence of the air. M. M. Dulong has shewn that the quantity of heat carried off by air is not only very large, but is governed by a law very different from that of ordinary variation; means have therefore to be devised for removing this cause of error; and I am far from thinking that the difficulty amounts to an impossibility. M. M. Dulong did succeed in determining the rates of cooling a body *in vacuo*, and I cannot see why others should not succeed in shewing the stationary temperature at one point of a body which radiates *in vacuo*; and this leads me to the following suggestions of experiments, by which I will conclude the present letter. First, as to the most important experiments. Second, as to the mode by which they may be conducted. Then, with a view of remedying the state of things to a certain extent, I will exhibit in their most simple forms some of the more obvious conclusions to which the different theories lead. No doubt much can be done in this way; but until called for, by the entry of experimenters on the field, a large and varied collection of formulæ would serve no useful purpose; the object being to discover a law of conduction: Then, as this can be best obtained by the selection of circumstances in which radiation plays no part at all, or in which its effect is very simple, and eliminated by selecting a small conducting power, such as a block of marble, and cooling it with a substance which will radiate very slowly, this experiment may be made upon a block of no very great dimensions, and, for many reasons, is well worth trying. I will point out another important experiment,—the determination of the state of temperature at one extremity of an iron bar, which is heated at the other; and if this experiment were made on a variety of bars of different conducting powers, and of different lengths, with a set of careful experiments of this nature, I am of opinion that the true law of conduction would be found, as I do not think the difficulties attendant on the conduct of the experiments to be at all insuperable. The real practical obstacle is the presence of the air, as I have proved that the law of cooling in air is different from that of radiation; even supposing, therefore, that we were in possession of the correct statement of that law, such would be the difficulty of obtaining formulæ from it, that to attempt to eliminate its effects, together with that of radiation, would be almost hopeless; if it can be done at all, it must be by means of experiments carried on in air of different elasticities; as it has been proved by MM. Dulong and Petit, that the velocity of cooling a body, due to the sole contact in gas, depends for the same excess of temperature on the density and temperature of the gas; but this dependence is such, that the velocity of cooling remains the same. If the density and temperature of the gas change in such a way that the elasticity remains constant, the effect, then, of the presence of air is to introduce a term which involves a power of the elasticity, as one feature, and a function of the excess of temperature as another: the latter function may be determined by means of a number of experiments, made at different elasticities; but I should greatly prefer a set of experiments on radiation *in vacuo*. But it appears to me that the difficulty in this case is the same which M. M. Dulong had to contend against in investigating the kindred law of radiation; and I conceive a similar contrivance to that which he used might be adopted to overcome it. All that is required is a certain portion of a bar, heated at one extremity, to radiate *in vacuo*, and that the temperature at two of its points, the other extremity being one, be capable of constant observation. A copper balloon could be made use of, similar to that used by MM. Dulong and Petit, which could be exhausted of air, and by means of ice be kept constantly at freezing temperature, notwithstanding the radiation of the heat from the body within. A somewhat similar contrivance, I conceive, would serve for the conduct of the experiment I propose. Then, let a bar of metal to be experimented upon pass through the balloon, and be heated in air, whilst the assumed point of heating might be marked with a thermometer inserted into a hole in the bar, just within the balloon; but this would have been much better, had M. Biott worked his lowest point not at the surface of the heated mercury, but a point a little above it: it would have insured a greater steadiness in the results. Should any one of your readers think of undertaking this experiment, I should recommend to his observation to examine a wider range of temperature than M. Biot has done. The thermometer which represents the heated end of the bar should stand permanently at every 5°, from 0° to as high as can be accomplished; the observations should likewise embrace a succession of bars of different substances,—iron, brass, lead, &c., all of the same dimensions. Different series of observations should be made, in which the dimensions of the bars have constant magnitudes, and others in which they have different lengths. All the substances might be coated with the same varnish, so as to render their radiating powers the same. With such experiments, I have no doubt that the law of conduction, though not, like the law of radiation, an inference from direct experiment, might be readily established, and the science of heat placed upon a footing with the other mathematical sciences.

JAMES WILLIAMS COLE,
Mineral and Land Surveyor, Bridgend.

A WALK THROUGH THE POLYTECHNIC INSTITUTION.

OUR country readers, many of them at least, will ask the question, what is the Polytechnic Institution? The best answer we can give is the plain English meaning of the word polytechnic; it is a compound from the Greek, signifying many arts,—and of a truth this institution is a collection of many, almost multitudinous works of art; and the exhibition of these, together with the lectures and the classes for instruction, form a means of imparting knowledge the like of which we know nothing of in the whole range of this kingdom.

But we have walked through it with a builder's eye, and thought as we walked of the effect that a Builder's Polytechnic School of this species would have, where the various specimens of natural and artificial products could be classified and arranged; where the processes in the superior departments could be explained and illustrated; where combinations of the beautiful in mechanism, in plain handicraft, and in the artistic section could be exhibited; where paintings, sculpture, carving, glass-staining, could be appropriately arranged; where drawings and models of ancient and modern works could be collected together, and drawings and models of the famous constructions, and indeed of the more simple, for the enlightenment of the artisan; where books, in fact a whole library of building literature, could be shelved and consulted; where lectures should be given directly applicable to building science, and many other things, all, one would consider, of manageable compass, as much so at least as this motley collection; and what is more than all, that it should be accessible to the workman at a very moderate rate.

We do not mean by this to say that the Polytechnic Institution has any thing in it so motley in character as to take the least exception at—it is perhaps, under present circumstances, the best technical gathering of a popular character that the public are disposed to receive and encourage; nor do we mean to say that the one-shilling charge for admission is one penny more than is right—in fact it is surprisingly moderate; nor, again, do we mean to say that it does not present a sufficient number of articles of a particular cast, so as to be of highly profitable interest to almost every class of artisans—perhaps, indeed, for present objects, it is the most captivating book of information into which the general public can be tempted to look, and it may have the effect, and undoubtedly has the effect, of turning the minds of thousands from desultory processes of research and investigation, and winning them, as it were, to a more critical mode and object of study and experiment; but after all, we cannot help wishing for that important following out, for the builder's behoof, which will give him what this gives to the more superficial and the merely curious inquirer.

In a previous number of this Journal the suggestion in part was thrown out by a correspondent, in reference to an architectural exhibition-room, independent of the Royal Academy, and we penned a few remarks upon that suggestion, which embodied most of what we have now given utterance to. We shall indeed be very ill-content until we see a step taken in advance towards this desirable and all-important end.

But how could it be expected that any such object could be carried out without, in the first place, a mouth-piece of the character of THE BUILDER? Our efforts, like ourselves, have been scattered and kept back for want of the one secret power, the organ of communication and publicity; but we proceed to our purpose; we would, however, venture to express a hope for the present that the Polytechnic collection could be enriched by a larger amount of building products, and an arrangement of readier reference; but above all, that some scheme could be devised for giving to the workman an access, privileged it may be, but at a price more within his means.

There are specimens, however, of many articles of exclusive interest to the builder: at our very entrance in the hall, on the right hand, we have a complete workshop in turning manipulations, and the curious in such matters may dwell here with profit on the various processes to which ivory, hard wood, and metal are here submitted, through the agency of two lathes, worked, as are all the machines in the hall, by steam power. We have here a model e-

To learn about PDF Compression and OCR go to ThePaperlessOffice.org